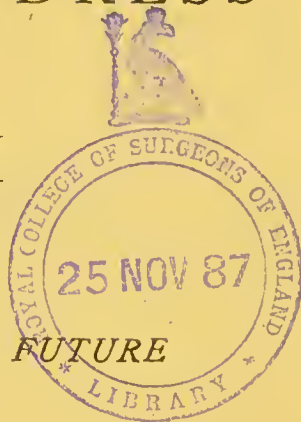


INAUGURAL ADDRESS ON LISTERISM

ITS PAST, PRESENT, AND FUTURE



BY

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ADDRESS ON LISTERISM :

ITS PAST, PRESENT, AND FUTURE.



GENTLEMEN,—My first and most pleasing duty is to express my sincere and grateful acknowledgment of the very high honour you have conferred upon me by placing me in this responsible position. Following in the footsteps of the two distinguished men who have preceded me, I am only too conscious of my own unworthiness of this eminent post, feeling sure that, for the efficient discharge of the duties of this chair, I shall have to rely on your kind assistance, while I must bespeak your indulgence with my shortcomings. If, however, the fact that I am animated by the strongest desire to promote the interests of this society, and, still more, to advance that branch of our art which this society has been established to foster, may be regarded as constituting any claim to your goodwill, then I yield to no man in this room, or indeed in our ranks. I will only add that I am determined to give my closest attention to the duties of this office, and to make every effort in my power towards maintaining the high position this society has already attained, and towards advancing the important objects we all have at heart, and for which we exist as a society.

When I look back on the addresses which have been delivered from this chair in the short period of our existence as a society, on the eloquent orations of our first President, as well as the practical and instructive addresses of my im-

mediate predecessor, I confess I am oppressed with the consciousness that it will be very difficult for me to maintain the high standard with which we have started, while I feel that this is, already, one of the instances in which I shall have to ask for your indulgence.

On casting about for a subject that would be worthy at once of this occasion and your attention, it occurred to me that I could not do better than bring before you the subject of Listerism : its past, its present, and its future.

I have selected this subject not only because of its intrinsic importance, but also because it gives me an opportunity of vindicating my own attitude towards it, of putting myself right with my professional brethren, and of removing some misrepresentations to which I have been subjected.

It is scarcely necessary for me to explain what constitutes Listerism, though, for the sake of completeness, it may be as well to do so as concisely as possible.

Listerism, then, is founded on the hypothesis that ordinary atmospheric air contains germs or seeds of disease, which, falling upon the field of operation, develop there, and in that development are answerable for the majority of the unsuccessful results of surgical practice. To prevent that development there was instituted the practice of charging the air surrounding the patient, and especially the field of operation, with a substance which was supposed to have the effect of destroying, or at least of neutralising, the influence of those germs. As you are all aware, the substance finally selected was carbolic acid. This was employed in the form of a spray to play upon the wound, and of a solution for the hands, instruments and sponges, in the proportion of $2\frac{1}{2}$ per cent., or 1 in 40. It is not necessary for me to examine minutely the theory on which this practice was founded. It may be summed up in the words of an eloquent writer in the *Times*¹ of a few years back, as follows, viz. : " This practice was based upon a hypothesis which was not proven, and is very probably not true." It need not, therefore, surprise us that, the pre-

¹ "Modern Surgery," the *Times*, April 1, 1880.

misses being wrong, the deduction should prove to be faulty. The correctness of the above dictum, however, will be demonstrated as I proceed in my argument.

LISTERISM : ITS PAST.

Many of you will remember with what trumpetings this practice was heralded forth, and how it seized not only upon the professional, but also the public mind, and bound it, as it were, in the spell of a new religion. It rapidly spread over this country and the Continents of Europe and America, and soon found its way to the remotest corners of the earth. And such were the faith cherished and the zeal displayed that, as has so often happened in the case of religion, even persecution of heretics followed in its train. Nor can we wonder at this result when we find a man, occupying the position of Professor Nussbaum, of Munich, closing a course of lectures, in 1880, with one in which he "discussed the consequences following to Medical Jurisprudence from the revolution in surgical opinion caused by the antiseptic method." So strong an adherent was he "of this method that he would extend the statute of the German Penal Code, dealing with bodily injuries and damage to health through negligence or malapraxis to such a case as that of a surgeon examining a wound with a finger not disinfected according to the strictest antiseptic principles." (*British Medical Journal*, January 8, 1881, p. 61.)

Hence, in the course of time, we read of an unfortunate midwife being consigned to prison, in Russia, because she had omitted to wash her hands in a solution of carbolic acid before examining her patient, though she had done so on leaving her own home; for to this omission the fatal result of the case was attributed. Nor have our own ranks been exempt, for, in Germany, "a surgeon was put on his trial for malpractice, and convicted on the ground that he had not observed the well-recognised rules prescribed by modern medical science for the treatment of such cases (the patient had been stabbed in the chest with a knife), and that he must

have known by ordinary observation that his own treatment would be followed by death"! Here the reporter adds a note of admiration, and then goes on to say: "On an appeal to a superior court this remarkable judgment was confirmed, and, although a further hearing has yet to take place before a third tribunal, the whole transaction suggests unpleasant thoughts of what might possibly arise in our own country if this example of the Teutonic method were to find followers on the English Bench." ¹

Again, a surgeon was committed to prison in Austria because he had treated a case of compound fracture of a limb without Listerism, and was not released until pressure had been brought to bear on the authorities by his professional brethren.

That a reaction should be the result of such exaggerated zeal as characterised the exponents and adherents of this practice was inevitable, and it was not long ere the theory, which, as I have said, rested on a false basis, was overthrown by the results of clinical experience, while it equally failed in its scientific aspect, as shown by the fact, demonstrated by Mickulicz, that bacteria lived even in a 10 per cent. solution of carbolic acid.

Nor were there wanting indications to show that a storm was a-brewing. Low murmurings, as of subterranean upheaval, were heard from near and from afar. Carbolic acid was known as a destructive agent when applied to living, however serviceable it might be in preserving dead, organic matter—as our breakfast tables abundantly testified. Men had begun to look out for another agent that might be free from its deleterious effects, and for a while salicylic acid and thymol were, in some places, substituted, but only to be given up in their turn. Then there was a further disturbance of the equanimity which had at first seemed to prevail, and this time of still more serious import; for one of the essentials of the system—as it was then regarded—was called in question. I mean the spray. Early in the year 1878, Professor von Bruns, of Tübingen, began a series of experiments directed to

¹ *Medical Times and Gazette*, November 22, 1884, p. 719.

the testing of the value of the spray. But it was not until December, 1880, that his results were published in a paper bearing the following title: "Fort mit dem Spray" (Away with the Spray); and the conclusion he arrived at was as follows: "The carbolic spray in surgical operations is not only useless and unnecessary, but also disagreeable and productive of interruptions; it should, therefore, be discontinued."

These murmurings of discontent were partial in their nature, but they were sure heralds of the impending storm. I omit any reference, for the present, to my own experience at this time, and to the change which took place in my opinions and practice in April, 1879, in order that I may give a continuous and connected view of the part I have played in this matter.

Many will remember how strenuously the late Mr. Callander opposed the Listerian method; but the first rude shock to which the system, in its entirety, was subjected, was in the memorable address of Mr. Savory, delivered before the British Medical Association at Cork in August, 1879. In that address, which threw such consternation into the ranks of the Listerites, Mr. Savory contrasted the results of a more simple treatment with those obtained by Listerism, and certainly not to the advantage of the latter. He also challenged the adherents and upholders of the system to publish their results, not in isolated cases, but in continuous series. His words on this part of the subject will bear repetition. He asks, "Are there any trustworthy statistics to show that, hitherto, the results obtained by Lister's plan are better than the best results obtained by any other method? . . . I take, for instance, our hospital statistics on the one hand, and—I seek in vain for any parallel results on the other. And, while this is so, I shall consider that I am justified in the conviction that, hitherto, the best results have been achieved by the simpler method. I must add, too, that one seeks in vain for statistics of any kind from sources from which, I submit, we are entitled to expect them. Why are such statistics withheld? Are they not worth the trouble of collection? And, if they have been collected, why are they not published?"

That was a true and correct statement of the case when Mr. Savory wrote. To the present day his challenge has been only partially accepted. Next in importance I place the paper of our late President, read before the Royal Medical and Chirurgical Society, on February 10, 1880, entitled "The Antiseptic Method Tested by One Hundred Cases of Successful Ovariectomy." In that paper Mr. Lawson Tait, while fully accepting the theory of putrefaction as applied to dead organic matter, denied its validity in the case of living organisms. I have read his paper again for the purpose of this address, and would commend it to you for perusal. Mr. Tait said :—" If the views of the germ theorists were correct, we ought to expect that no operation could be done successfully without rigid antiseptic " (in the sense, I presume, of Listerian) " precautions. . . . There ought to be no difference in the mortality of operations in small and in large hospitals, in town and in country. In fact, if germs could have had the unbounded influence which is claimed for them by many antisepticists, surgery must long ago have been an extinct art, if, indeed, it ever could have struggled into existence." He called attention to " three factors upon which the uniform experience of operating surgeons has taught them that the success of their work depends," these three factors being, viz., " the condition of the patient, the condition of his surroundings, and the nature and extent of the operation performed." And he adds, " Of these three, most undoubtedly the most uncertain factor is the first," viz., the condition of the patient—a statement with which I entirely agree.

But though, as I think, Mr. Tait conclusively maintained his position as far as he went, he fell, at that time, short of the logical conclusion, because he had not yet emancipated himself from the influence of the prevailing *fashion* of the day. For he concludes with these words : " My suspicions are fully confirmed by the facts I have given above, and, though they will cause me to modify my conduct of the details, especially in the dressings, yet I shall continue to employ all the antiseptic precautions (Listerian precautions) during operations,

as I have hitherto done, until a wider experience decides whether they may not all be given up."

I thought, at the time, that that conclusion weakened the force of his argument, and I think so still.

However, we now know how far that modification of the details has since been carried, until, in the process of evolution, as was believed by Lord Monboddo to have occurred in the case of the human species, "de tail" has entirely disappeared! We know also with what results.

About the same time, that is, early in the year 1880, appeared the very important correspondence on surgical statistics between the late Professor Spence and Mr. (now Sir) Joseph Lister, in which I believe it was generally admitted that the victory remained with the former. Nor must I omit to remind you of the remarkable "Results of Surgical Treatment without Antiseptics in the Kilmarnock Infirmary," published by Dr. MacVail in the same year. In this instance, one of the essential peculiarities of the practice was in direct contradiction to the Listerian theory and practice; for, as in the case of an amputation of a limb, the flaps were exposed to the air for several hours, or until all oozing had ceased and the raw surfaces had become glazed over. Here, then, was a grand opportunity for the entrance of germs! But they seemed to refuse the offer.

The year 1880 closed with my own paper "On Hyperpyrexia after Listerian Ovariectomy," read before the Royal Medical and Chirurgical Society on December 14, to which I shall have to refer further on.

But the following year was still more disastrous to the pretensions of this system; for it was in that year (1881) that the *coup de grâce* may be said to have been given it, at the meeting of the International Medical Congress held in this city, when Dr. Keith announced that he had given up the spray. Our late President may perhaps remember the remark he made to me as Dr. Keith sat down: "Listerism is dead." This was a crushing blow; for great had been the jubilation over Keith's eighty consecutive cases of ovariectomy

without a death. But we have Keith's own authority for saying that pure Listerism played no important part in that magnificent record; and he further told us that in his next twenty-five cases, done under the same system, he had a mortality of 20 per cent. Listerism had now received a shock from which it has never recovered: the spray rapidly disappeared from the operating room, and has now become almost an object of antiquarian interest.

It will now be convenient to refer more at length to my own part in the history of this question.

Listerism may be said to have been in full swing in general surgery ere it was deemed prudent to apply it in abdominal surgery. I believe Keith was the first to employ it in this department of our art. He was not satisfied with his first essay, but again took it up with the result we now know.

It was introduced into the practice of the Samaritan Free Hospital by Mr. Knowsley Thornton, in the autumn of 1877. In the beginning of 1878 glowing reports came over from Germany as to the superiority of thymol over carbolic acid, for which it was substituted as less irritating and equally efficacious, and I performed my first operation with this substance, after the Listerian method, on February 13 of that year. On March 13, 1878, I performed my sixth ovariectomy in this manner, and the result was fatal. That was not promising. Then I resorted to Listerism pure and simple, using absolute phenol, and paying the greatest possible attention to all its details. My second case, done under this system, died of acute pulmonary congestion, while everything within the peritoneum was in a perfectly satisfactory condition. Was this due to the chilling effect of the spray? My fourth case died from septicæmia. It was a case of very broad attachment, requiring nine ligatures. Nowadays I would enucleate the tumour in such a case, and probably drain. The fact, however, remains that death was due to septicæmia. Not to weary you with too many details, I will summarise the results up to my thirty-sixth case (the seventy-second of my total number) by saying that out of the thirty-six I lost eight—giving

a mortality of 22 per cent. as against 26 per cent. in my first thirty cases, and 20 per cent. in the six cases done with thymol. I will not trouble you with an analysis of these cases further than to say that in the first thirty cases five out of the eight deaths were due to causes over which the Listerian method, with all its pretensions confirmed, could not be expected to exercise any control: that the one death under thymol was due to septicæmia, and that of the thirty-six Listerian cases one died of genuine septicæmia, and in three the death might fairly be attributed either to the chilling effect of the spray or the poisonous effect of the carbolic acid. In my thirty-sixth case the patient had been tapped ten times, the tumour weighed 51 lbs., and was adherent to the whole extent of the anterior abdominal wall, as well as extensively to the omentum. Within twelve hours the temperature rose to over 103 degrees, and in spite of repeated sponging with iced water it steadily went up to over 107 degrees; the patient became delirious, and no urine was secreted. By means of the ice-pack the symptoms were subdued, and at the end of twenty-four hours the urine again began to flow, at first in very small quantity and afterwards abundantly. Suffice it to say that the patient, in the course of her convalescence, presented all the characteristic symptoms of acute nephritis, such as hyaline casts, albuminuria, indican in large quantity, &c. This case threw a flood of light on the thirty-third case of this series, a fortnight previously, in which the patient died with somewhat similar symptoms in twenty-six hours. *Post-mortem* examination revealed acute congestion of the kidneys as the sole apparent cause of death. I was now on the horns of a dilemma; for if, as was contended by the advocates of this system, I was protecting my patients from septicæmia by the use of carbolic acid, I was at the same time exposing them to danger of poisoning by the self-same substance. From that time, therefore, I began to reduce the proportion of carbolic acid in the spray and solutions to 1 in 50, 1 in 60, 1 in 80, and lastly to 1 in 100, which I reached in July, 1880.

The results of that long-continued experiment I communicated to the Royal Medical and Chirurgical Society, in the paper already referred to. In it I gave ample evidence of the effects of carbolic acid upon the kidneys, and of its action in the production of hyperpyrexia. But I will only trouble you with the concluding paragraph, which is in these words: "It only remains for me to add that I have now proved that the claim that has been set up for Listerism (in preventing pyrexia after operations) cannot be sustained; that carbolic acid may be introduced into the circulation in poisonous quantity by means of this method, that when so introduced it manifests its presence by producing a state of hyperpyrexia, and that thus it actually produces, too often, what it was intended to prevent. And the practical result of all this, so far as I am personally concerned, is, that, to meet the evil which to me is so formidable, I have gradually diluted my spray and solutions, so as to reduce the whole question to one of *cleanliness*, which, after all, is the true secret and merit of Listerism, and, I am happy to say, with the effect of very greatly adding to the success of my ovariectomy work." We shall see how far that conclusion has been justified by events. This aspect of the question was further dilated upon in my reply on the discussion.

The first result of the reading of that paper was a reply by my own colleague, which partook more of the nature of an independent statement than an attempt to question or explain away my results and conclusions—a statement which was sadly marred by personalities, and has since been followed up by ever-increasing personal hostility.

I was also told by my friends that it was unwise on my part to go against the stream; that by opposing Listerism I was running counter to popular as well as medical prejudice, and that I was doing myself a professional injury. But, gentlemen, I called to mind the old adage, *Magna est veritas et prevalebit*, and as I was determined to follow, at all hazards, what I sincerely believed to be the truth, I resolved to carry out my experiment to its ultimate results, and until I should

be either convinced of my error, or confirmed in my faith. That I suffered I am free to confess. It is within my personal knowledge, and what, think you, was the first visible effect? The President of the Royal Medical and Chirurgical Society at that time, viz. Mr. Erichsen, thought I ought to belong to that society, and under his ægis I allowed myself to be nominated for the Fellowship. I was supported by the cream of the surgical branch of the profession in that application, and it is humiliating to think that the united voice of those men was of so little weight that I was *twice* rejected. Who were my opponents? It is not improbable that the majority of the men who succeeded in bringing about that result, even by a bare majority, were little known beyond the circle of their own firesides. Little did they think that they were paying me the highest compliment in their misdirected power. Had my name been known only as that of a legally qualified practitioner I should, no doubt, have passed the portal without a dissentient voice, without even the click of a solitary black ball. And rumour even has it that I need not have looked far afield from my own hospital in search of the prime instigator of that intrigue.

A much more discreditable thing remains to be mentioned, but in this instance I have authentic information as to its author.

In the *British Medical Journal* of May 20, 1882, there appeared a leading article under the title of "Simple and Aseptic Ovariectomy." I would observe, in passing, that the term "aseptic" is a misnomer. An operation cannot be called *aseptic* until the result is known; therefore the operation itself is said to be *antiseptic* when means are employed with the view of preventing *septic* mischief. I would also remark that the name of Spencer Wells occurs with suspicious frequency.

The author of the article begins by referring to two papers recently published: the first by Professor Kocher, of Berne, "On Ovariectomy in Switzerland," and the second by Dr. Yandell, entitled "On Ovariectomy by Thomas Keith, M.D."

In the first place the writer compares the results of ovariectomy before and after the use of antiseptics, in the Listerian sense, in Switzerland, and he quotes Professor Kocher to the effect that the total mortality in 300 cases was 23 per cent., and that while the mortality in the first series (of 55 cases) was 63 per cent., that of the second (245 cases) was only 11 per cent. "Going on to the experience of individual operators, Professor Kocher shows that the six surgeons who have done the largest number of operations, and have operated both before and since the adoption of the Listerian precautions, 'have diminished the mortality one-half since their use of antiseptics;' and he argues that it is a very strong argument in favour of the antiseptic treatment that it has enabled twenty-three different operators, some of them with very small experience, to arrive at a result superior to that which Spencer Wells attained before the method of Lister" (and, he might have added, which he has not been able to improve upon by the adoption of the method). "Putting aside a rare, exceptional case of death from some accident—collapse or obstructed intestine, for example—nearly all the deaths are from peritonitis, which means infection. A death from peritonitis is, therefore (in Professor Kocher's words) 'a reproach to the operator;' for we are now, by antiseptics, enabled certainly (observe this) enabled *certainly to avoid peritonitis as a sequence of Ovariectomy.*"

Time does not permit me to join issue with the writer on this subject, but I would ask the question, Has peritonitis, then, been banished from the death-roll of those who practise the Listerian method? *Most certainly not.*

The next paragraph is chiefly devoted to the purpose of showing the remarkable accordance of Professor Kocher's antiseptic precautions with those of Spencer Wells, and of quoting the views of the latter as to the "chilling effect" of the spray, his doubts concerning it, and effect of the "misty cloud" in obscuring the field of operation, closing with this argument, that "comparative observations made under similar conditions with and without spray were required

before we could ascertain what is the share which the spray, among other additional antiseptic precautions, has had in obtaining the better results, which have undoubtedly accompanied their combined employment." "It is," he says, "interesting to find that Professor Kocher has given up the spray in private practice as 'injurious in the conditions where it is not indispensable'"—whatever that may mean.

Then the author of the article in question passes to what he calls "the very remarkable experiences of Keith, as recorded by Yandell." He points out that in Keith's results "the mortality was steadily diminishing all that time" (i.e. in the pre-Listerian days). In the first eight cases under antiseptics there were two deaths, and then there was a run of eighty cases without a death. And finally, "Since leaving off the spray, and including twenty-six other cases done without it, Dr. Keith says, 'I have had but a single death in a total of fifty-two cases done without antiseptics.'"

But the sting is in the tail, as it should be in accordance with Nature's law in the insect world.

He goes on to say "All this affords ample material for serious reflection. If in Switzerland antiseptics have been followed by a remarkable diminution of the death-rate, if Dr. Keith had one run of eighty cases, treated antiseptically, without a death" (it will be convenient to omit all mention of a mortality of 25 per cent. in the immediately preceding eight cases, and of 20 per cent. in the immediately succeeding twenty-five cases), "is an occasional death from carbolic acid poisoning (which may, perhaps, be avoided by not using solutions unnecessarily strong, or by the use of some other agent)"—a case of begging the question—"to justify a surgeon in operating without more than ordinary protection against infection? What does recent experience in London say on this point? Has the mortality after ovariotomy in our large hospitals been smaller of late years than it was before? If so, can the diminution be explained by the increasing experience of the operators? On this point, the registrars of our general hospitals may give some important evidence; and in

the special hospital which may still be regarded as the headquarters of ovariectomy—the Samaritan—the experience of the year 1881 should afford an array of important facts. In that hospital the patients are very much of the same class of life, and are treated under very similar conditions by three operators of large special experience.” (The writer could not even state this part of the case correctly, for one of the operators had only begun to operate in January of the same year ; but it may be stated, to his credit, that he got through the year with eight cases without a death.) “Two of these surgeons,” he continues, “use the spray of carbolic acid in every operation, of a strength of 1 in 40, and follow out in the strictest manner all the peculiarities of the Listerian method. The third, after gradually diminishing the strength of the spray till water alone was used, gave it up altogether. Here, then, were the ‘comparative observations made under similar conditions with and without spray’ proposed by Mr. Spencer Wells, carried out under singularly favourable conditions. And what has been the result? We have been informed that the Committee has expressed a strong opinion against the performance of ovariectomy, for the future, without full antiseptic precautions ; and the experience of 1881 would seem to justify this recommendation, however objectionable it may be to admit of any interference by a committee of laymen in practical details. Still, if in the same institution, at the same time, by operators of equal experience, the same operation with antiseptics is followed by a mortality of about 7 per cent., and without antiseptics is followed by one of 30 per cent., the fact deserves very serious consideration, and is rendered additionally important by the apparently contradictory results of experience in Switzerland and in Edinburgh which have now led us to direct the attention of our readers to the matter.”

Need I say, gentlemen, that the third operator here referred to is the man who is now addressing you ?

In the first place, you will observe how careful the writer is to conceal his identity. Now, the Committee of the

Samaritan Hospital consists of the *consulting medical officers* and a certain number of laymen, and when he suggests how "objectionable it may be to admit of any interference by a *committee of laymen* in practical details" he evidently desires to conceal this fact. It was the duty, then, of the medical to direct the deliberations of the lay portion of the committee. Must I ask you to believe that the facts necessary to guide them to a just decision were actually placed by myself in the hands of the writer of this article, as one of the medical members of the committee, and the most authoritative amongst them? It does not stand to reason that the medical was overruled by the lay element in such a matter as this. Hence we are driven to the conclusion that the resolution, if it did not meet with the concurrence of this most important member, did not encounter his resistance. He must, therefore, be held to be *particeps criminis*.

Nor is it an exaggeration to characterise the summary of results as a tissue of misrepresentation. It is not merely an error of calculation which any schoolboy would have avoided; for, as I have said, the exact figures and facts were placed in the writer's hands. It would be a charitable assumption that a man who was unable to perceive the true bearing of his own facts, as, for instance, when comparing the results of the clamp and cautery respectively, could not be expected to interpret rightly the facts and figures that were supplied to him by another. But this is not a case for the exercise of charity, and we must find a more simple explanation.

What, then, were those data? They were these, viz., that in the year 1881 I performed thirty-five ovariectomies in the hospital, of which seven died—giving a mortality of 20 per cent. instead of 30 per cent., as stated by the writer of the article. But how did these patients die? I answer: Two died from hæmorrhage (slipping of pedicle, &c.), two died from shock within ten hours, one died from obstructed intestine on the seventh day, and two died from what I will call septicæmia, and one of these died through the too early

removal of the drainage tube, i.e., while the serum was still red. Thus of the seven cases two only have any bearing on the question under discussion—yielding a mortality of 6 per cent.

That is the way in which this important question was presented—I ought, perhaps, rather to say *misrepresented*—to the profession. That was the way to analyse “comparative observations made under similar conditions with and without spray,” in the judgment of the author. What do you think of the performance? What do you think of the Author? Have you read the riddle of the Authorship?

Well, gentlemen, it is scarcely necessary for me to say that I paid no attention to the “strong opinion” of the Committee under these circumstances; but, believing I was in the right way, continued to dispense with “full antiseptic precautions”—a phrase which we now know only meant Listerism—paying the strictest attention to *cleanliness*. And what is now the result? The result is a total mortality in my practice since that time of 7 per cent. But more of this in its proper sequence.

Listerism was still the order of the day, though its adherents were gradually falling away one by one. It is a remarkable fact that our late President and myself were, without any intercommunication of ideas, working in the same direction, at the same time, and with the result that we arrived at the same conclusion—a fact which I scarcely need say is very gratifying to me.

Statistics were, however, being appealed to with the view of demonstrating the superiority of the Listerian method. We all know how statistics may be manipulated. One glaring instance of this must not be passed by. Many of you will remember the animated correspondence on this subject two years ago, between Mr. Lawson Tait and Mr. Knowsley Thornton, evoked by the latter's announcement that he had just then completed seventy-five cases of ovariectomy, in the Samaritan Hospital, without a death; and you will remember that Mr. Tait challenged Mr. Thornton to produce

his private results. That challenge was not accepted. Why? Because, as we now learn from his recently published table, that mortality was as high as 13·5 per cent. That was at least a *suppressio veri*. But the same table furnishes us with yet another instance of the same practice. Mr. Thornton contended, in his recent paper, that his results showed a progressive improvement. Well, what does his table tell us? It tells us that in the first hundred cases the mortality was 9 per cent., in the second 5 per cent., but in the third 7 per cent.

How is this difficulty got over? Simply by dividing the 300 cases into two groups of 150 each. By that means he arrived at 7 per cent. for the first group, and 6 per cent. for the second. Is that a fair way of stating the case? Is that sufficient ground for asserting progressive improvement in results? I will not weary you with further examples.

I am now about to do what I have myself denounced in the practice of my chief opponent—I may say my only opponent. I have denounced the practice of going to the register of a hospital and abstracting the results of a colleague's practice without that colleague's consent, and before he has himself published the results, or they have been published by the authorities of the hospital. Why do I do the very thing I have myself denounced? Because I do not choose to allow my opponent to fight me at an advantage.

Well, what does the year 1886 tell us? It tells us that Mr. Thornton has had thirty-two cases of ovariectomy, of which six died—giving a mortality of 18·7 per cent., while all of mine—twenty-five in number—have recovered. How will the writer of the article to which I have called your attention interpret these facts? Let me apply his own words. If, in the same institution, at the same time, by operators of equal experience, the same operation *with* antiseptics is followed by a mortality of 18·7 per cent. in the hands of one operator, or 15·2 per cent. for all the Listerian ovariectomies, and *without* antiseptics is followed by *no mortality at all*, the fact deserves very serious consideration. I will not pursue the

parallel. Nor should I omit to mention the fact that for the last three years (1884-5-6) the mortality in the Samaritan Hospital *with* antiseptics is 10·6 per cent., while that *without* is only 4·2 per cent. Such, then, is the irony of fate! I care not now to inquire how all these patients died, because I deem it only fair to mete out to others the measure that was meted out to me. True, that is a most unscientific manner of dealing with such a subject. A bare statement of results, as, for instance, of the number of cases with the number of deaths, affords no information on such a question as this, and it behoves us to be very careful how we draw deductions from them. Yet that is what was done in my case, and you see the result. It would have been better for the writer of these articles to have let them alone. Where now, I ask, is the evidence of progressive improvement?

I will not weary you with more than a passing reference to an article in the *Medical Times and Gazette* of the same date, evidently written by the hand of a man in sympathy with this system; for such was his infatuation that it was a question with him whether a death within ten hours should not be regarded as due to what might be called *express* septicæmia—galloping would scarcely indicate the speed—rather than shock. Nor is it necessary to do more than point out that the explanation which was appended to the list of deaths amongst my cases in the report of the Samaritan Hospital for that year, and which was called in question by the writer of this article, is now stamped with approval by the acceptance of the selfsame nomenclature by those of my colleagues who still practise the Listerian method in all its rigidity. It would be a waste of time to combat his arguments, for they are beneath contempt.

With the statement that of my last 100 cases, bringing down my results to the first week of this year, the mortality is only 4 per cent., that of the last sixty of these cases only one has died, and that the last fifty have all recovered, I close this long, and, I fear, too wearisome reference to my own work. I offer no apology for the length of my statement,

because I am sure you will allow that a full explanation is of vital importance to me in view of the misrepresentation under which I have suffered.

LISTERISM : ITS PRESENT.

I now proceed to direct your attention to the present position of the Listerian question. Wherever we look, at home or abroad, we find indisputable evidence of waning faith in this system. That faith has undergone all degrees of modification, from the strongest conviction to total disbelief in its efficacy—nay, even to a belief in its injuriousness. There is not a general hospital in this country where it has maintained its vitality. Even in its transplanted home in this city it is conspicuous by its absence. This we have on the authority of Dr. Playfair, who told us, on October 26 last, that he had “recently operated on five cases in succession at King’s College Hospital without spray ; indeed, on one occasion when he asked an assistant to prepare it he was told that there was not a spray-producer in working order in the hospital” (*British Medical Journal*, December 25, 1886, p. 1280). In a few isolated places, however, the spray may still be seen, as, for example, in the Samaritan Hospital, where it is employed by my colleagues in all its pristine purity of detail ; sometimes it may be seen playing on the back of the operator, sometimes on the backs of the spectators—all of whom may not have had a bath recently—and sometimes as a means of laying the dust previous to the commencement of an operation.

But it is to Germany we must look if we desire to see the farce in full swing. In Berlin it is the habit of one of its most distinguished gynæcologists to operate in a room which more resembles a wash-house than a surgical operating room, with its “misty cloud” of carbolised steam obscuring the field of operation, and its walls and floor streaming with carbolised solution. And when visitors are admitted they are required to give an assurance that they have had a bath in the morning

—a very wholesome thing surely—and are obliged to divest themselves of coat, waistcoat, collar and cuffs, all this in an atmosphere so damp that they soon look and feel as if they had been taking a bath, and so irritating to the air passages, that “all in the room are more or less inconvenienced from it, and are compelled to cough a great deal” (*Cincinnati Lancet and Clinic*). Why all this elaborate extra-precaution against infection if the operator believes in the efficacy of the system? To my mind it is the most convincing evidence of want of confidence in it.

It was stated as a fact by Mr. Thornton in 1880, that “as a rule there was no fever at all after an antiseptic ovariectomy, and, as Mr. Wells had said, ‘the ice-cap was now never required.’” Let us consult Mr. Thornton’s last table again: and what do we find? We find that of the 300 cases the temperature remained under 100° in only twenty cases, that it remained under 101° (but over 100°) in ninety-four cases only, and that it exceeded 101° in nearly two-thirds of the whole number—or 186. What, then, is the limit of fever temperature?

But I will contrast what that table tells us with my own results. I take his last hundred (i.e. bringing his cases down to January, 1886), and my last hundred, viz., to December, 1886, and this is the result:—

Highest temperatures in Mr. Thornton's last 100 cases	Highest temperatures in Dr. Bantock's last 100 cases
Under 100° . . . in 10 cases	Under 100° . . . in 19 cases
Over 100° , but under 101° . . . in 37 „	Over 100° , but under 101° . . . in 48 „
Over 101° . . . in 53 „	Over 101° . . . in 33 „
Not exceeding 100° in 8 „	Not exceeding 100° in 15 „

Giving Mr. Thornton the benefit of his most successful 100 cases, viz., with a mortality of 5 per cent., we get the following result:—

Under 100°	in 8 cases.
Under 101°	in 39 „
Over 101°	in 53 „
Not exceeding 100°	in 1 case.

Observe the uniformity in the numbers in both instances, under the same heads, except the last. That is very remarkable, and indicates a very uniform average amount of pyrexia in "antiseptic ovariectomy." Note also, that whereas he had only 47 cases with a temperature under 101° , I had as many as 67. Note further, that while he had only 1 and 8 cases respectively in which the temperature reached but did not exceed 100° , I had as many as 15. Could I have desired any more confirmatory evidence in support of my contention that carbolic acid, or the whole system if you will, produced a state of hyperpyrexia? That is the kind of evidence on which is based the assertion that "as a rule there is no fever at all after antiseptic ovariectomy." The fact is, the rule is just the reverse. See how dangerous it is, sometimes, to publish tables!

Nor can it be claimed that I derive benefit, from the employment of Listerism by my colleagues, in keeping the hospital free from septic germs; for, while cases have been dying in other wards from more or less pronounced septicæmia, mine have escaped under a system in which all antiseptics are banished from the operating room. I have not even deemed it necessary to hang up a wet carbolised sheet over the doors of my wards in these circumstances. On the other hand, what do we again find in Mr. Thornton's table? To his 365th case he appends this note: 'Had septic abscess in abdominal parietes. Patient in opposite ward died of acute septicæmia.' Now for the facts. I operated on my patient in the morning, and Mr. Thornton on his in the afternoon of the same day. There was subacute peritonitis in my case, and the patient died on the fourth day. It may be assumed that Mr. Thornton did not once expose his patient's wound till after my patient had died. But, further, the wards are situated at the top of the house. They are, moreover, separated by the breadth of the intervening staircase or well of the house, and are distant from one another over ten feet. The skylight is *always* open, and at that time of the year (July) the door leading to the roof, immediately over the

landing between the two wards, is also for the most part open. Thus, there is the freest possible supply of fresh air into the well, to say nothing of the direct ventilation of the ward from the opposite outside wall of the house. Let it be granted, now, for the sake of argument, that his case was infected by mine, then I ask what is the use of his "full anti-septic precautions"? But is it not much more rational to suppose that Mr. Thornton so closed the wound as to leave an opportunity for bloody serum, and perhaps liquid fat, to accumulate between the raw surfaces?

Such is a sample of the evidence on which this system is bolstered up.

Not only was fever abolished, but even "the need of the drainage tube had passed away." So said Mr. Thornton in 1879, and as late as 1884. Is this so? His table tells a very different tale.

Yielding under the ever-increasing weight of accumulating facts tending to negative the Listerian theory and practice, it is now said by those whom I will call its apologists that, after all, it is the *cleanliness* ensured by the system—that which, in 1880, I ventured to characterise as the true secret and merit of Listerism—that has produced such an improvement in surgical results. If this be granted, then Listerism has been sailing under false colours. Why not, then, accept, in all its simplicity and innocuousness, the principle which is thus admitted to underlie it, and to which it owes its success? I see no way of escape from this alternative. Even Mr. Thornton, who has been the most zealous and combative disciple of the Listerian School, who would have us believe that under the protecting influence of the spray fever has been abolished, that septicæmia "has been removed from the causes of mortality," and that "the need of the drainage tube has passed away," who seems to have retained his faith in the system after his master has lost his, and who has taken every opportunity, in season and out of season, of thrusting his "full antiseptic precautions" down our throats, even he now tells us, in words that may be regarded as intended to antici-

pate a possible charge, that he "is no bigot in the matter of the spray," but that it is merely "a useful element," for "it keeps everything about the wound constantly moist with an antiseptic material"; and, further, wonder of wonders! that he is even "prepared to admit that washing out the wound with water is very valuable." (*Lancet*, October 20, 1886, p. 818.)

And how do matters stand, at the present day, as to the general results of Listerism, when brought into comparison with those obtained under the more simple method at which Mr. Tait and myself, with many others now following, have arrived? Where is there any record in its history to approach Mr. Lawson Tait's marvellous run of 139 consecutive cases without a death? What has Mr. Thornton to show in support of his boast that he would "yet beat Keith on his own ground"? His recently published table tells us that his longest run is 48. Thus he stands fifth on the list, which runs as follows:—

Tait, with 139 cases.		Bantock, with 50 cases.
Keith, „ 80 „		Skene Keith, „ 49 „
Thornton, with 48 cases.		

And this, too, at a time when, if ever, he may be supposed to have mastered all the details and perfected the system. I will say no more as to his latest results. But nowhere is there to be found more convincing evidence in support of my contention than in the practice of Sir Spencer Wells, who has not improved his results by even 1 per cent. over those obtained in the hundred immediately preceding his adoption of the Listerian method.

Nor will I leave room for the suggestion that, perhaps, all these fifty cases of mine were simple; for I had to resort to the drainage tube twenty-eight times, in nineteen cases the operation involved both ovaries, and in one of these the uterus was also removed at the level of the internal os, in only sixteen cases were there no adhesions or their equivalent, in about one half of the cases I had to wash out the peritoneal

cavity, and in one case the patient had just completed seven months of pregnancy.

There is another aspect of the question which has been very much overlooked in all discussions on this subject. We are told by microscopists that the orifices of the mucous canals—such as the alimentary and generative tracts—swarm with living bacteria. These are the organisms, which, a few years ago, were supposed to work all the mischief. But their reign is over, and we are now living under the dynasty of the bacillus. Whatever be the name of the mighty potentate who keeps us in thralldom, his sway, it appears, does not extend to those prosaic regions. Who has ever seriously proposed that we should operate with “full antiseptic precautions” upon a ruptured perineum? or a vesico-vaginal fistula? or a hare-lip? or a cleft palate? And yet, what do we find taking place under the very eyes, so to speak, of this “destroying Angel?” Why, this, viz., that in all of these instances perfect union by first intention almost invariably takes place, provided due coaptation of the raw surfaces be maintained, provided the sutures be not drawn too tight, and the parts be kept at rest. Even at the commencement of my Listerian practice, and while entertaining a belief in the system, I was puzzled by the facts I have just mentioned. And when I first ventured to question the value of the system I was politely told, by the man to whom I owe so many compliments, that I did not understand the subject.

There are some who still maintain that the spray and Listerian details must be employed even in amputation of the breast to obtain the best results; and the time even was when we were asked to strain our credulity to the extent of believing that a breast case had broken down because the spray had failed for a minute or two, or had been, by a whiff of wind, driven from the wound for a moment in the course of the operation, or even in the subsequent dressing, or because there was a pin-hole in the mackintosh outside the 8-fold gauze dressing! This was making a demand upon our credulity with which, it is to be hoped, for the sake of our

common intelligence, few have been prepared to comply. I challenge any adherent of this system to show better results—more rapid healing, or less irritation of the wound—than I obtain in these cases without any antiseptic whatever.

I cannot refrain from closing this part of my subject with the quotation of a passage from Froude's "Life of Carlyle" which in its aptness will be found to be very striking. The biographer is referring to Ruskin's "Letters on Political Economy." I will substitute *Listerism* for "Political Economy," and *Surgical* for "Political" and the passage will read thus:—*Listerism* "had been a creed while it pretended to be a science. Science rests on reason and experiment, and can meet an opponent with calmness. A creed is always sensitive. To express a doubt of it shakes its authority, and is therefore treated as a moral offence. One looks back with amused interest on that indignant outcry now, when the pretentious science has ceased to answer a *surgical* purpose, and has been banished by its chief professor to the exterior planets."¹

LISTERISM : ITS FUTURE.

Finally, Gentlemen, what is to be the future of this system? Shall I play the *rôle* of prophet, and attempt to forecast its future? The old adage forbids. Perhaps, ere many years have gone over our heads, *Listerism* will already have become a thing of the past and as a tale that is told; perhaps the men of even the next generation, in the course of their study of this subject as matter of "ancient history," will be heard asking of one another the question, "What was it all about?" And perhaps it will be chronicled as one of the crazes to which, to our humiliation be it said, our profession had been given up soul and body. And as we now smile at the doctrines of the Rationalists and the Empiricists, of the Dogmatists and the Methodici, as we are lost in wonder and amazement at the belief in charms and amulets, and in the

¹ *Life in London*, vol. ii. p. 244.

efficacy of the royal touch, and as we ridicule the vagaries of the alchemists and the astrologers, of the mesmerists and the healers by faith and prayer, as well as the antics of the African medicine-man, so will our sons, perhaps, smile at the credulity of their fathers, and wonder at their unreasoning faith in the virtues of a practice which "was based upon a hypothesis that was not proven," and certainly was "not true."

